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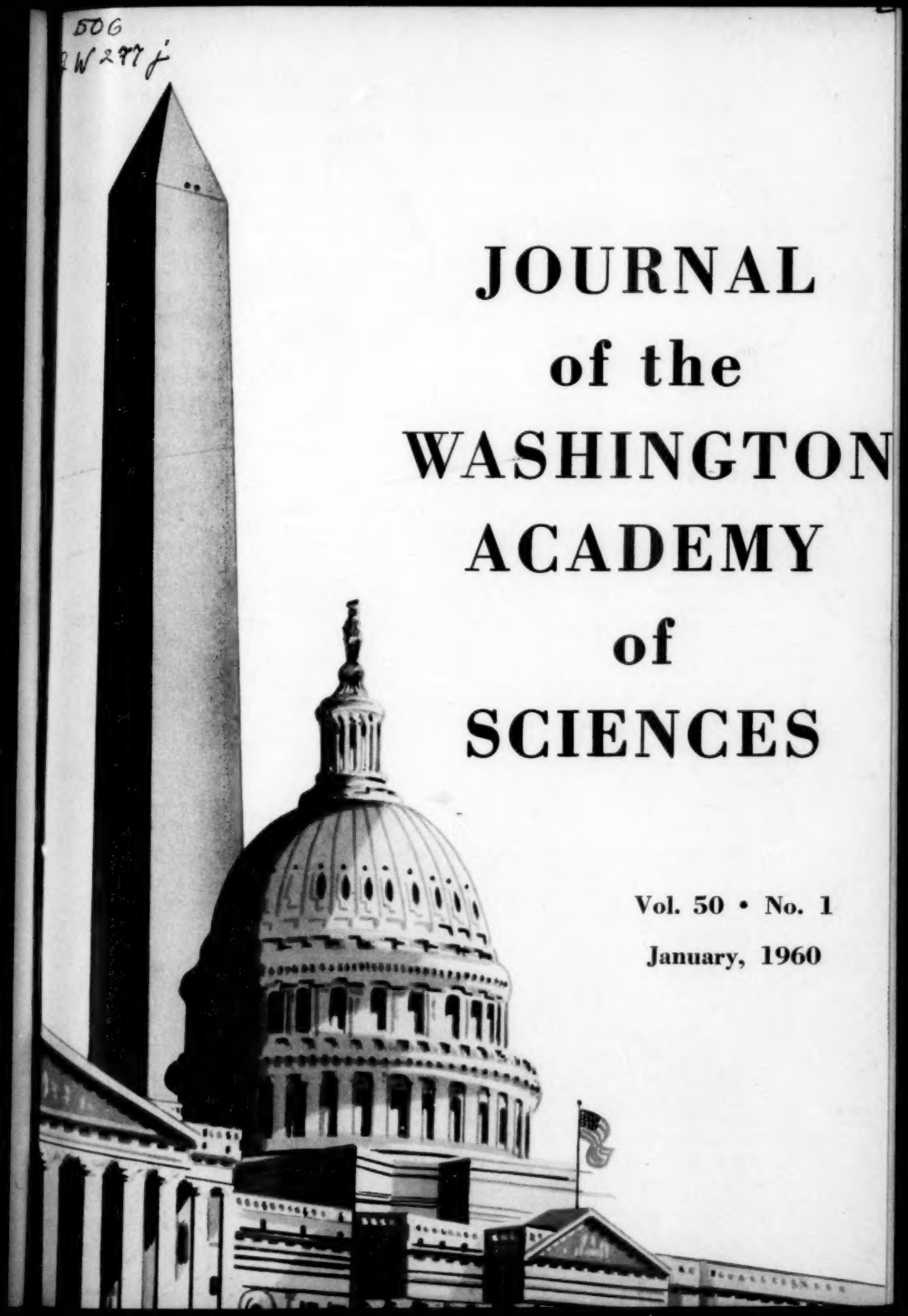

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The Journal for 1960

Frank L. Campbell

All members of the Washington Academy of Sciences would probably agree that its *Journal* should contain whatever the majority wants, within the bounds of financial and human feasibility. If the Board of Managers can be regarded as a fair sample of the membership, it is certain that the majority does not want the kind of *Journal* we have had in recent years, but we still do not know what the majority does want. It is also possible, but improbable, that the Board does not represent the opinions on the *Journal* of the membership as a whole. Because of these uncertainties the Board at its December meeting authorized a referendum on the question of the contents of the *Journal*. We must act now on the probability that changes in content are desired, but if the referendum shows that the majority wishes to support an archival *Journal* such as we have had, we can and will promptly revert to it. If it shows that the majority do not wish to support such a *Journal* but are not sure what they do want, then we should experiment with the contents of the *Journal* guided by comments we hope to receive from interested members. It will be much easier for members to offer suggestions after they have in hand something to criticize.

The new editorial group has only one conviction at this time; namely, that the *Journal* should serve in part as a medium of communication between the officers and members of the Academy, and between the Academy and its affiliated societies. But some members disagree even on this point. They fear that the use of the *Journal* as a medium of local communication may cause it to become essentially a house organ and submerge or extinguish its scholarly aspects. This feeling is prevalent not only among those who favor an archival *Journal* but among those who will not give up hope that the *Journal* could become a medium for the publication of papers reporting original research in both experimental and descriptive science. Experience shows, however, that such a hope is not realistic. Only a *Journal* having lavish financial resources for free and prompt publication could hope to draw all kinds of high-grade research articles away from specialized professional journals. We doubt that such a hope could be realized even under optimum conditions because experimental scientists have a strong desire to publish their most significant results in journals that are seen regularly by their own professional colleagues.

It seems to us that local communications in the *Journal* need not destroy its scholarly character. If the *Journal* can no longer be a factor in the publication of primary research, it should and can publish its share of papers concerned with the later processes in the growth of knowledge—what Paul Weiss calls the digestive and assimilative processes leading to the production of knowledge. Such papers may be called historical articles, critical reviews, critiques, etc., papers that bring together existing information and opinions and integrate them into the body of knowledge. Members of the Academy have ideas and opinions to publish in the *Journal* that will be of interest to scientists everywhere. We think such feature articles will take up much of the space in the *Journal*. They should be articles that every member of the Academy and other scientists can read with interest and with profit. Not all such articles will have permanent value, but they should give to the world examples of the synthetic writing of Washington scientists.

Some members contend seriously that there is no need for a *Journal* that can be read with interest. They argue that we already have more general publications

than we can read and that the Academy can make its best contribution to science by publishing an archival *Journal*. There is much to be said for this point of view. It is true that we all have too much to read. We guess that few scientists could state that they read every issue of *Science* from cover to cover, and yet we think most scientists like to feel they are paying for a periodical that may contain something they want very much to read, and when something extraordinary comes along, the word travels rapidly on the grapevine; for example, "Have you read in *Science* Warren Weaver's proposal on special committees?"

If the referendum indicates that our members want a *Journal* they can read if they wish, then we can go ahead toward producing a *Journal* that more and more of them will want to read. This first issue from the new editorial group is the best that could be done under present difficult circumstances. Subsequent issues should become progressively more comprehensive and interesting as the volunteer organization needed to produce such a *Journal* becomes experienced and effective. Please remember, however, that dues were not increased for 1960 and that we can spend this year, for the production of the *Journal*, only $4000, less than half as much as we did in 1959. Therefore, the *Journal* will be smaller than it was last year and the possibilitities for improvement will be restricted until greater support can be provided. We hope that you will be patient and tolerant as we move toward a *Journal* in which the Academy can take new pride.

Virchow's "Cellular Pathology" in the Framework of Biology and Medicine*

Alfred Plaut

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This title is chosen because the centenary has brought the words "Cellular Pathology" to the attention of many. The topic at hand, however, is cellular physiology and cellular histology as well; it is cellular biology. The terms theory and doctrine also are used without intending to establish a standpoint a priori. Virchow himself liked the neutral term cellular principle.

When his lectures were published in the Fall of 1858 with the title "Die Cellularpathologie in ihrer Begruendung auf physiologische und pathologische Gewebelehre" the words "cellular" and "cell" had been used for more than a century. One is tempted to marvel at the prophetic vision of the poet when one reads Voltaire's question, written about 1755.[1] "In what corner of cellular tissue lies the genius of Homer and Virgil?" But, cellular tissue was in the 18th century what since 1834 has been called connective tissue, and it was supposed to contain the vital force. Blumenbach, the father of anthropology, saw in it the seat of the "nisus formativus", the tendency to create; for Caspar Friedrich Wolff, the pioneer of embryology, it harbored the "vis essentialis" the real life force. An analogous misunderstanding has made Lamarck a precursor of the cellular principle. The old meaning survives in the term "cellulitis" used for an inflammation in subcutaneous connective tissue.

* This article is based on a lecture given at the Armed Forces Institute of Pathology, on November 12, 1958, as part of the Institute's Virchow Centennial Program.

An 800 page American pathology book of 1845,[2] speaks about "cellular transformation", meaning transformation of another tissue into loose connective tissue, which the author calls "cellular tissue". The word "cell", as far as I can see, is mentioned only casually in the book.

When things seen under the microscope in sections of cork were called "cells" for the first time (Hooke, 1665) the name was given on account of the thick rectangular walls. It is a far cry from there to the still valid definition of a cell as "a lump of protoplasm with a nucleus".[3] We reckon the cellular era from the year 1839 when Theodor Schwann in Berlin published his "Microscopic Investigations on the Accordance in the Histological Structure and Growth of Plants and Animals". The thesis, implied in the title, that not only plants but animals also are composed of cells had been voiced before by several people more or less clearly but had failed to make an impression on the scientific world. Dutrochet in 1824 had declared the cell "the fundamental part" of the animal organism.[4] It is difficult to know how far his "cells" and those of other observers in the late 18th and early 19th century really were cells and not starch granules, droplets of some substance, or artifacts caused by the chromatic aberration of the lenses and the lack of condensers. The often used term "globules" makes one doubtful, and it might be wise not to accept "cells" as such unless they are unambiguously described or illustrated.

Johannes Mueller, the teacher and spiritual father of a whole group of men whose names are connected with the development of cellular ideas in Germany (Schleiden, Schwann, Virchow, Remak, Henle) recognized the cellular composition of tumors in 1838. Purkinje in Breslau, who was ahead of his time in several respects, had perhaps the clearest ideas about cells before Schwann. The good observations of Goodsir in Edinburgh were recognized by Virchow himself. (The second edition of the Lectures on Cellular Pathology is dedicated to Goodsir.) Virchow's publications begin to show his preoccupation with cells in 1847, in the first volume of his "Archiv", but at this time he still believed with Schwann and others that cells were formed in an amorphous substance, the "blastema". Later he shared Remak's [5] view that cell division is the only source of cell formation. Much about cells had been written by others, and much he had said himself in his lectures and written about them during his professorship in Wuerzburg (1849-1856). One can understand therefore that he was astonished about the success of the book, saying that it did not contain much that was new. He wanted to ascribe the effect to its easy style. As it appears today it was more the exhaustive, convincing presentation and the offer of a promising method.

The change caused by the "Cellular Pathology" was perhaps even greater in France than in Germany. Cruveilhier was professor of pathology in Paris until 1866, his atlas of gross pathology is unsurpassed in beauty but he did not use a microscope. One of his pupils [6] went to Berlin and studied under Virchow together with four Scandinavians. He transplanted modern pathology into France. As he said many years later, Virchow's teaching had been a revelation, pathological anatomy became alive, it became connected with physiology. The difference between medical teaching before the "Cellular Pathology" and afterwards was perhaps greatest in Russia. The influence on pathology in England was characterized as replacement "by orderly arranged and objective facts of what was before a tangled wilderness of old superstitions and modern disconnected observations". Similar opinions were voiced at the celebration of Virchow's 80th birthday "from Archangelsk to Cadiz", not to forget Japan, and the United States.

William Osler, with his historically trained mind, realized that such a revolution in medicine could not be ascribed to one man, that the "Zeitgeist" worked like a leaven. "But", he continued, "no physician of our time has done more to promote the change, or by his individual efforts to win his generation to accept it

than Virchow". The cellular theory stimulated people to make use of the improved microscope. As long as a man believed that alterations of body fluids or disturbances in innervation were the essential factors in disease he could not expect to learn much about disease by looking into the microscope. But when he expected to find meaningful alterations of cells then he had reason enough for using this lately improved gadget.

The English translation of the cellular pathology [7] appeared in 1860, and within a few years, seven American editions were sold out, in spite of the fact that pathological anatomy always has been a stepchild in the United States. Before 1865, that means in the years when Virchow's interests were essentially in pathology, American students went to England and France rather than to Germany, thus Virchow had no important direct American pupil. Welch, the nestor of American pathology, worked under Virchow's pupil Cohnheim, whose experiments had laid the foundation for modern concepts of inflammation. In Welch's laboratory at Johns Hopkins Hospital, Virchow's postulate of combining morphology and experimental pathology was fulfilled to a degree but it is tempting to speculate how American pathology might have developed if Welch had worked in Virchow's institute. The unprecedented impact of Virchow's publication—Welch said it was the greatest advance ever made by scientific medicine—must have had several causes. One of them might have been that it offered a striking fact of which people were not, or not sufficiently aware, namely the cellular composition of the body, and at the same time a unifying principle. People suddenly were made to realize that the human body was composed of cells like any other organism, and that human diseases were disturbances in the life processes of these cells. In the middle of the nineteenth century, the human being was to most people something apart from the rest of creation, and diseases were entities apart from the body. The new teaching thus catered to man's desire for unified concepts.

Important progress in medicine is generally attributed to technical inventions, and impulses given by changes in theoretical concepts are overlooked. The transition of tumor surgery from knife shyness to optimistic activity in the later decades of the nineteenth century was undoubtedly caused by the emergence of anesthesia, antisepsis and asepsis. But about at the same time, cancer became a local disease in the minds of physicians while it had been a constitutional one before. And what is the use of performing a local operation for a constitutional ailment?

The much discussed topic of the precursors to Virchow and the cellular doctrine may be dealt with by quoting what Sir Gavin de Beer recently has written about Darwin: "For various reasons, including imperfect formulation of the problem and insufficiency of evidence, none of these precursors was able to compel attention, let alone adherence, to these views; and it is because of the completeness of his demonstration . . . that the world owes its debt to Darwin".[8] Claude Bernard and internal secretion furnish another parallel. The idea of internal secretion existed in the mind of Albrecht von Haller, almost a century before Bernard's publications; several people voiced similar opinions, and in 1849 A. Berthold reported a successful substitution experiment by implantation of testicle into a castrated rooster.[9] None of these workers gave internal secretion to the scientific world, but Claude Bernard did. There are more points of comparison, in spite of the fact that the "Cellular Pathology" did not shake mankind as the "Origin of Species" did. Both represent the synopsis of painstakingly accumulated observations, many of which were made after the general idea had been conceived. Both ran counter to feelings that had been dear to man since time immemorial: Darwin offended the idea of man's exalted position in nature. Virchow hurt the human individual's proud feeling of being a truly indivisible entity. As Virchow

said, all plant physiology was based on the cellular principle, and the reluctance of people to introduce the concept into animal physiology was based only on esthetic and moral objections. Darwin as well as Virchow may have had a deep-seated personal feeling for their theories. Darwin [8] in his notebook from the Galapagos Islands (1835) calls animals "our fellow brethren in pain, disease, death, suffering and famine, our slaves in the most laborious works, our companions in our amusements," and he continues "they may partake of our origin in one common ancestor—we may be all melted together." His just conceived theory obviously agreed with his love of animals. And through Virchow's whole life, there goes the idea of true democracy in which every individual feels as a participant in a harmonious whole, namely the state. He often talked about the "Zellenstaat", he called the body "a free state of single beings with equal rights," and continued, "they keep together because the single parts are dependent upon each other." A few years later, he used the term federation to designate the relation of the cells to the body as a whole. His antimonarchial, democratic heart made him overdo such comparisons; he loved them. When we read what he said in parliament (1867) about decentralizing the administration of the state . . . "the administration to be based on the freedom of the communities . . . the constitutional life built from below . . ." [10] then we almost expect to find something about cells in the next line.

When we draw etiological conclusions from histological pictures, we remember that the patterns in which a tissue can react may be few, and that diverse causative factors may result in one and the same picture. This is well exemplified in dermatology. But one should not forget that the number of ideas a rational human being is prone to conceive about a group of phenomena is restricted also, and that identical or closely related ideas spring up independently in different minds. Most of us enjoy having the good idea and let it go at that, genius, however, puts it to work. This historical reasoning applies to some of Virchow's ideas which can be found in various writings of the German Romantic period, and I hesitate to assume from this a philosophical underpinning of Virchow's theories. We know that he liked to read Aristotle but it would be difficult to prove that his idea of the cell state stems from Aristotle's concept of the body as a well-governed commonwealth, neither do I know whether he ever read Kant's "Critique of Judgment" in which a similar comparison is made.

The history of cellular doctrine through the century of its existence is in no small measure a comedy of errors or misunderstandings. Virchow insisted that he had based everything on observation, and that criticism could come from observations only. Others maintain that his work had its basis in philosophical ideas of the Romantic school. Virchow saw the main merit of his work in abolishing systems and speculation, but Ricker called the cellular pathology anthropomorphistic nature philosophy.[11]

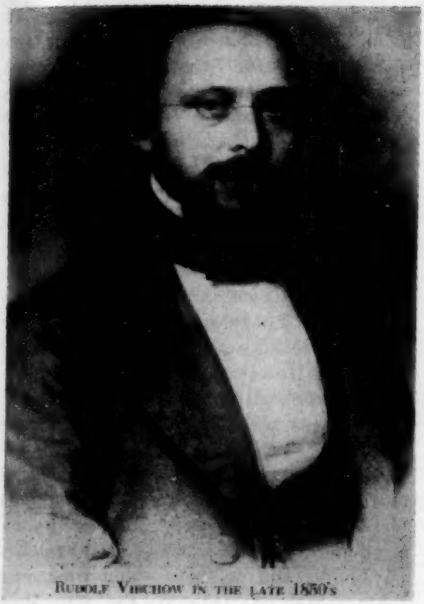
An impressive almost incomprehensible foreshadowing of cellular theory may be seen in the words of Goethe,[12] who was a biologist in his own sublime way. "Every living thing is not a single thing but is a plurality. Even when it appears to us as an individual it still remains an assembly of independent living beings, that are equal according to the idea, to the anlage, while in fact they may appear equal or similar, unequal or dissimilar . . . The more imperfect a being is, the more equal or similar are these parts and the more they resemble the whole. The more perfect the being becomes, the more dissimilar will the parts be. The subordination of the parts indicates a perfect being." And he was conscious of both the analytic and synthetic standpoint when he wrote to Alexander von Humboldt: "Your observations start from the element, mine from the gestalt."

Virchow was proud that his doctrine was purely scientific, not burdened with a system of therapeutics as the old "schools"

were. But Klebs, Virchow's brilliant, rebellious pupil said that cellular pathology could not amount to much since it had not even created new therapeutics. Virchow insisted from the beginning that it is not so much the morphology but the life of the cell that counts; nevertheless cellular pathology, even today, evokes in some people the idea of one-sided, "dead" morphology.

The history of cellular doctrine in the framework of biology can be studied by considering the standpoint of those who oppose it for scientific reasons or otherwise. Virchow's pupil Cohnheim, who by his famous experiment proved that leucocytes migrate through the capillary walls, has been considered an opponent of cellular theory by the medical historian Baas and by others. In reality Cohnheim only refuted Virchow's erroneous ideas that the blood vascular system is entirely closed and that all pus cells are formed in loco, but he was not opposed to cellular theory. In his lectures on general pathology, he calls the cells "living beings with a metabolism . . . which certainly is a very active one in many of them." Thus I rather agree with Cameron that Cohnheim's experiment gave a new impetus to cellular pathology. It showed cells, namely the leucocytes, in action, while they are not in contact with other tissues.

The most vigorous attack upon cellular pathology was made by Gustav Ricker in 1924. He wanted to replace it by "relation pathology." He denied that the cell has any life by itself in health or disease, he did not believe that the cell has a choice in taking substances out of the surrounding blood. Virchow's idea that the cell has the faculties of function, nutrition and proliferation appeared metaphysical to him and an obstacle to clear, scientific causal thinking. The cell, he said, cannot be looked at in any way as separate from its relation to the central nervous system and to the blood. He denied even the anatomical concept of the cell, pointing to syncytial formations like the myocardium, the fetal part of the placenta or the skin epithelium with its intercellular fibres. The functions and diseases of the body, Ricker concluded, after decades of purposeful animal experimentation, are located in the central nervous system. It governs everything by means of the nerves that accompany all blood vessels, even the smallest ones, and that form delicate networks around and in the cells. The cell does nothing, according to Ricker, things are done to it. A statement made by Virchow in 1855 represents a powerful argument against Ricker's theory. He wrote that an entirely general innervation of the parts is not sufficient for explaining "the many special happenings in the course of nutrition and formation." The restriction of special happenings to structures as small as cells could be explained only by isolated and qualitatively different effects exerted by the same nerve. And this, Virchow continues, "is contrary to all available experiences." [13] Unless I am mistaken the second part of this argument still holds while the first one has been superseded by the discovery of nerve endings in many cells. What happens in the organs, rest and function, anabolism and catabolism follows, according to Ricker, a definite law, his "Stufengesetz." Constriction and dilatation of vessels, which are regulated by the central nervous system through the vasomotor nerves, are the essential factors. Morphological findings and localization in the sense of Morgagni, Rokitansky and Virchow are not of primary importance, according to Ricker they are only one item in diagnosis. The whole body is the seat of disease. In Ricker's opinion, Virchow's anatomical thinking has been proved infertile by the history of medicine. Form is to Ricker no scientific reality but a concept of reflexion that can induce only philosophical thoughts without heuristic value. (To avoid misunderstandings, it might be inserted here that Ricker considered Virchow's merits immortal). Similar thoughts have been brought forward in 1934 by Speransky,[14] a pupil of Pavlov. He believed that he could produce most known pathological lesions by local freezing of cerebral cortex, by withdrawing and



RUDOLF VIRCHOW IN THE LATE 1850's

reinjecting spinal fluid repeatedly, or by squeezing the hypothalamus into a glass ring. He acknowledged the achievements of cellular pathology in systematizing pathological processes morphologically but he doubted its value for constructing a dynamic theory. Not only Virchow's ideas but also the medicine of Pasteur and Ehrlich was approaching exhaustion in Speransky's opinion, and could not cope with the contradictions that have arisen. Speransky's book makes fascinating reading and has influenced some people, notably in Germany, to a degree designated as "Speransky psychosis" by others. Attempts at repeating his experiments however, were successful only when the dogs were infected with leptospira.[15] Ricker had, and still has, followers, but more and more people declare themselves unable to confirm his "Stufengesetz" which is supposed to regulate the bloodstream after an irritation has taken place. And it has been shown long ago that nerveless tissue of vertebrates can react to a stimulus.[16] I agree with those who think that Ricker's work is mainly of historical interest today, and few people, I trust, will share the opinion of Claudius Mayer that Ricker's ideas will become as important for the future development of medical research as was Virchow's cellular theory for the second half of the 19th century.[17]

Theories and doctrines are essentially a matter of emphasis. Ricker, the most important enemy of cellular pathology, knew everything that was to be known about cells but he stressed the dominating influence of vascular innervation. In the same way, Virchow knew that the organs are subject to changes in blood supply, and are innervated, though the finest nerve endings were not known in his time but he minimized that because he was so enraptured by the cells. Nobody denies that Virchow sometimes exaggerated in this respect, but one could quote numbers of his utterances that show appreciation of the body as a whole. And Ricker's theory neglects the many life phenomena of organs and cells that are separated from their nerves and from their normal blood supply. Even the complex Goldblatt phenomenon, the rise in blood pressure after reduction of arterial flow to one kidney, can be produced when the kidney first is transplanted to the neck, which entails a drastic disturbance of perivascular nerves. In all such discussions, we forget too easily that plants function as organic units without having a nervous system. Opponents of cellular theory who stress the nerve conditioned totality of the metazoan body often refer to Pavlov. A survey of Pavlov's writings, however, does not indicate that he was opposed to cellular theory. He used expressions like "the struggle for existence among the cellular elements," he calls the cell "a whole organism, if a small one;" he talks about disturbance in cerebral cells as cause of the behavior of an animal; he lets cells have "capacity for work," mentions "relaxation of nerve cells" and "weakened irritability of gastric cells."[18] Speransky's ideas had been accepted in Russia for a while but have been repudiated lately.

The teaching of pathology, histology and embryology at medical schools has not

been significantly influenced by the objections raised against cellular doctrine. Numbers of recent textbooks, English, French or German, teach according to cellular doctrine, mentioning the theory or simply taking it for granted. E. R. Long in his history of pathology wrote in 1928: "We are all cellular pathologists today, taking our post Virchowian cellular sense for granted." [19] A recent French textbook of zoology and a German text on botany also stress cellular structure, almost unreservedly as did the botanist Kuester in a historical survey of the problem in 1938.[20] Such an attitude must be expected because histology and pathology can neither be practised nor taught from the holistic standpoint, no matter how much one might be enamoured of it philosophically or esthetically. Horst Oertel, in his General Pathology (1921) emphasized the interrelations of cells whose disturbance by a local process might be of more consequence than the local process itself, and that means disease of the whole organism.[21] The holistic totality aspect and the atomistic (cellular) one are the two horns of a dilemma out of which there is no escape for the pathologist or any other biologist. The poet-biologist Goethe has worded it succinctly, in 1832, a few weeks before his death: [22] "Thus one is driven from the whole to the detail and from the detail to the whole whether one wants to or not." If we stress the heuristic standpoint, we can say that to consider the whole body as the main object of investigation means replacing one riddle by another one; but the man who is essentially interested in the patterns of nature will strive again and again to comprehend the totality of the organism without getting lost in too much detail. An author who with a subtle logical distinction, expresses doubt whether pathology of the cell really means "cellular pathology" declares in the same paper that the cellular doctrine is, today more than ever, one of our secure possessions.[23] He even thinks that Virchow's much attacked cellular ideas about inflammation have more of a future than the accepted vascular concept of inflammation as established by Cohnheim. A philosophically minded pathologist, wrote in 1928 that pathology is not pure cellular pathology anymore, which does not mean that it is wrong, but that pathology has unending variations and thus cannot be deduced from one single principle. Roessle, the third successor in Virchow's chair, characterized Virchow's concept of the body as "a state of autonomous, widely independent cells," and formulated his own as "non autonomous single cells cooperating with each other and with blood and the nervous system." I feel that Roessle in this exaggerated Virchow's emphasis of the independence of the cell, but what with the multitude and variety of Virchow's statements, this remains a matter of personal taste. Many authors have an ambivalent attitude towards the cell concept because on the one hand they see the cells, their role in physiology and pathology, and work with them all the time; but on the other hand they see the body as a whole and are unwilling to give up its individuality. An example for this is furnised in a modern English textbook of pathology.[24] It regards the cell as the ultimate unit of structure and function however complex the tissue or organ may be. But, it continues, "it is more profitable to regard the organism as the individual, . . . and the cells not as units of which it is built up, but rather as parts into which it is divided in order to provide for the necessary division of labor. (Adami [25] expressed the same standpoint in 1910). The conception of the cell thus remains, but no longer requires or is capable of the strict definition that was needed when the word was supposed to represent a fundamental biological entity." One should note the words "in order to provide," in the second half of the statement. This is teleology and shows the danger inherent in the biological totality concept. When the authors say "it is more profitable to regard the organism as the individual," I agree with them from the psychological standpoint but I have doubts from the standpoint of biological methodology. Hueck, whose studies of the

mesenchyme have played an important role in the discussions about the cellular doctrine, considers the totality concept as absolutely necessary but, he too adds that we can study only the parts.[26] The titles of publications are revealing. The first volume of the handbook of microscopic anatomy (Moellendorf) which appeared in 1929 bears the title: "Living Matter", thus referring collectively to the substance of the organism and ignoring the cell, while the corresponding volume of the handbook of general pathology in 1955 is titled: "The Cytoplasm," which means the stuff cells are made of.

Our tendency toward accepting one opinion and condemning the opposed one smacks of man's primitive fighting instinct, and he who objects to a theory is prone to exaggerate it because that makes it vulnerable. Opponents of the cellular doctrine have invested it with a primitiveiy additive character neither Virchow nor his followers had seriously in mind. Thus we read in a modern textbook of histology: "Every cell of the human body, according to Virchow, is a kind of homunculus, and the combined efforts of these well specialized microhumans shall result in the preposterous cell state that represents the organism." [27]

This book leaves out the word "cell" in its definition of tissue; it uses the words "elementary parts" and "masses." The author tells us that "function can be conceived only dependent upon the undefinable totality of the organism." Since this gives us no hope of understanding this totality why not stick to less hopeless problems, reserving the totality concept for fields in which we can use it, or for a remote future? At the other end of the line we have the statement made by a zoologist in 1957: "The living part of nature is an atomized system; its atoms are the cells." [28]

The period of rising bacteriology and immunology brought many attacks on cellular pathology which were mainly based on lack of knowledge in both fields. A French author wrote in 1885: "Cellular pathology has lived. Down with the cells." The ideas of sera, of circulating antibodies brought a resurgence of some kind of "humoral pathology." Deeper insight, furthered by Paul Ehrlich's work, showed how such substances are manufactured by cells, and today we see no contradiction between immunology and the cellular doctrine.

Internal secretion, in health and disease, has been considered as an expression of the total organism and as contradicting the cellular concept. But since we know that giantism, acromegaly, and other endocrine disorders are caused by alterations of hormones, and since the hormones are produced by cells, sometimes by relatively few cells, the phenomena of internal secretion might be marshalled as well in favor of cellular doctrine as against it.

While cellular doctrine was under heavy attack the science of the cell was started and grew. The term cytology seems to have been used first in England about 1885; a magazine "La Cellule" was founded in Belgium in 1884; Verworn [29] wrote his much maligned book on general physiology in 1895. Today we have an imposing monograph, Cameron's "The Pathology of the Cell" (which in spite of its title contains much criticism of cellular doctrine), in the same year Caspersson [30] published his "Cell Growth and Cell Function." And what would Virchow say if he could see the title "The Mammalian Cell as an Independent Organism?" [21]

The way tissues are formed by cells suggests that the cells are morphological and functional units. The existence of syncytia, continuous masses of protoplasm with many nuclei, seemed to contradict the universal applicability of this statement. Opponents of cellular theory have written much about these structures,[32] but without agreeing among themselves, and in the opinion of some, including myself, not with convincing power. Structures with two or more nuclei had been seen in 1802 long before the true cellular era [33] and purley syncytical organisms, mycetozoa, were described in 1860 (see Baker). But, to my knowledge, no highly complex plant or animal is entirely or even essentially composed of syncytia. A crustacean

(Peroderma) has been mentioned as having no cellular structure, but this is a parasitic animal, and its lack of cellular organization is a secondary adaptation to parasitic life. Related species consist of cells, as other crustaceans do, and we can reasonably assume that the free living ancestors of Peroderma were cellular, not syncytial.

The absence of visible cell boundaries apart from the fact that our optical methods may be at fault does not necessarily indicate absence of functional boundaries. The botanist Sachs, in 1893, postulated smaller functional units in syncytia, each dominated by a nucleus, he called them "energids." This concept, which has not gained much recognition, appears plausible when nuclei are evenly distributed; it is convincingly demonstrated when two flagella are found near each nucleus. The idea that syncytial structure prevails in the metazoan body has led to describing the body as "a lump of living matter." [32] Others stress especially the syncytial nature of the mesenchyme (Hueck). The continuous character of connective tissue had impressed Virchow himself. He wrote in 1862: "The body appears composed of a more or less continuous mass of connective tissue like parts, as Reichert has pointed out." [34] The decision whether to call something a group of cells or a syncytium is sometimes an arbitrary one because cells which have distinct outlines are often connected by bridges or fibrils. Every medical student learns about the intercellular bridges of the skin epithelium. But it is more than doubtful if these fibers are biochemical pathways at all and actually make a syncytium out of the skin epithelium. Our daily experience as pathologists speaks against it since we see severely altered single epidermal cells surrounded by normal ones or near normal ones. Schultze, who in 1861 defined the cell as a lump of protoplasm with a nucleus, denied that even broader connections interfered with the individuality of cell life.[8, 33] The fibrils which seem to connect the epithelial cells of the skin may well have a mechanical function. It is noteworthy that these so-called intercellular bridges do not seem to interfere with the isolating of cells by microdissection. The concept of the cell certainly is not abolished by the existence of connections,[35] and syncytial structures are the exception in the essentially cellular metazoan organism. But anticellular considerations based on the textbook concept of syncytial structures begin to be only of historical interest. Electron microscopy has shown cell boundaries in most so-called syncytia, first in the neurons, the very embodiment of continuity, then in neuroglia, and lately also in heart muscle, skeletal muscle, nervous endplates, myoepithelium of glands, and squamous epithelium. This marks, "the beginning twilight for syncytial theories in which the lode star of cellular doctrine shines more brilliantly than ever." [36] Oskar Hertwig has called the cell "one step in the organization." With the progressive disappearance of syncytia, this step gains in importance.

The supposed active and decisive role of noncellular mesenchymal ground substance in the genesis of collagen diseases did not agree with cellular theory. But the factor that characterizes the classical collagen disease Lupus erythematosus disseminatus (the L. E. factor) seems to counteract an enzyme which normally inhibits depolymerization of nucleoproteins. This enzyme (desoxyribose nuclease inactivator) is found in cells, and provided these relatively recent findings will stand the test of time, the cellular doctrine will be vindicated once more. A similar situation exists concerning the formation of collagen fibrils. They can be formed in a solution of collagen molecules without the help of cells. But the collagen molecules in turn have been secreted by cells.[37] This knowledge, arrived at by our most modern techniques, was theoretically anticipated long ago. Adami wrote in 1910 "we can well admit, with Virchow, that the cells discharge living molecules of the order of enzymes which act upon and modify the surrounding matrix." At least two other supposedly cell free structures have been believed to be "living:"

The vitreous of the eye forms hyaluronic acid but as has been shown recently cells are numerous in its outer layers.[38] The second example is more striking. In fishes and in other animals, a long filament, which appears structureless on ordinary microscopic examination, extends from the brain deep down into the central canal of the spinal cord; it is called Reissner's fibre, its function is unknown. Older literature stated that the distal portion of this fibre could regenerate itself after transverse cutting, and this distal portion is not in contact with cells while the proximal is. Recent work however does not confirm this; the peripheral portion disappears after transection, and secreted material accumulates cephalad to the cut.[39] No metabolism that is independent of cells has been demonstrated to my knowledge. Johannes Mueller wrote in 1838 about the cellular composition of tumors, a year before Schwann's studies appeared, and since that time the tumor cell has been the center of onocology. The aggressive cancer cell evokes the image of an animal more vividly than the normal tissue cell does. Uncounted work hours have been spent in anatomical, physiological, and chemical laboratories with attempts at determining its characteristics. The practical diagnosis of malignancy in the hospital laboratory, however, has been based not so much on the characteristics of the single tumor cell as on the structures formed by tumor cells and their behavior toward the adjoining normal tissue, in short, it is more structural than cellular. Only in the last decades, following the work of Papanicolaou, has cytological tumor diagnosis gained momentum, and exfoliative cytology has become a recognized branch of clinical pathology. Combined with the hoped for progress in histochemistry this may develop into an important diagnostic method based on the characteristics of single cells. The failure of all the ingenious methods devised for a humoral diagnosis of cancer might be referred to in this connection. Developing in a time when cellular pathology was vigorously attacked, exfoliative cytology has, wittingly or not, furnished a prop for it.

Theoretical cytology is expanding rapidly, urged on by modern methods, and even the time hallowed, often belittled routine procedure of studying the paraffin section has added something to the biological importance of the cell, namely the sex chromatin. The single cell that bears the hallmark of as decisive a general body character as sex is, may claim a higher dignity than that of a brick in the building.

The cell is the only component of the metazoan body that also exists as an independent, motile organism, namely the protozoon. Neither organs nor tissues, nor any postulated units have a free living animal as counterpart, and the few existing syncytial organisms are poorly differentiated ones like the myxomycetes. This may be the strongest claim the cell has for being called "elementary organism," a title conveyed upon it not by an anatomist, but by the physiologist Bruecke in 1861. Another physiologist, Max Verworn, declared it inadmissible in 1895 to call a structure an elementary unit unless we knew about an analogous free living organism.[29, 8] The unicellular character of many protozoa is evident beyond doubt, and while complicated organelles and skeletal structures give others an organismic habitus they still remain a mass of protoplasm with a nucleus, and thus a cell. The cellular nature of bacteria has been proved, and viruses as well as bacteriophages, which are not cellular, are not free living organisms either.

The converse question namely how far the cells of the metazoan body have the nature of living organisms is much more difficult because their variety is so great and because "life" and "living organism" are ill defined concepts. Something halfway between the two problems may be seen in the behaviour of the cells of certain sponges, primitive sessile metazoa. When separated by squeezing the sponge through a porous cloth, they swim around with ameboid motion and sometimes reunite to form another sponge. This remarkable

sponge experiment did not create much of a stir perhaps because sponges are such lowly creatures. But, as has been shown recently, even mammalian embryonic kidney cells, when carefully separated, can reassemble and be organized into typical kidney structures.[57] This almost exemplifies what Oken wrote in 1805, "Organisms are a synthesis of infusoria." A generation later, Schwann went to the core of the problem when he wrote that each metazoan cell could lead an independent life if the relations it bore to its new surroundings were but similar to those in which it stands in the organism.[40]

In dealing with this problem, namely the autonomy of the cell, we must differentiate between the potential autonomy and the actual one. The former can be proved by single cell culture, e.g., but the latter is beyond accurate proof. I cannot imagine a method for proving autonomous behaviour of a cell while it is in its normal place within the metazoan organism. This is a kind of Heisenberg situation. Virchow, while repeatedly referring to the importance of neural and vascular influences, stressed the individual reaction of the cell very much, sometimes in an exaggerated way. Ricker, who believed that all reactions in the body are regulated by vasomotor nerves, denied any reactivity of single cells. Many opinions between these extremes have been voiced, and no general answer can be expected when we compare the dependent status of a ganglion cell with the relative freedom a leucocyte. During phagocytosis the leucocyte must breach its cell membrane and reestablish it after having engulfed the foreign particle, and it accomplishes this tricky physicochemical feat without being in contact with other tissue not to mention a nerve.

The power of locomotion is one of the criteria of life, but it stands to reason that metazoan cells, caught as they are in the tissues, cannot move around much. Leucocytes move; the term "wandering cells" speaks for itself; each type of myeloid cell has is own characteristic style of ameboid motion, and so has the lymphocyte. Motility of tumor cells has been observed long ago, and migration of nontumorous mammalian cells occurs in the pituitary gland of man, where basophile cells of the intermediate zone are seen singly invading the posterior lobe. I have little doubt that these cells are actually migrating.

As said before, one must differentiate between the actual and the potential motility of the cell. The latter must be widespread since all cell types in tissue cultures exhibit ameboid motion.[41]

The double nature of metazoan cells manifests itself in the tissue culture. Mammalian cells, isolated by trypsination, can be grown in serum free media.[42] They move around, more or less in the fashion of amoebae, single cells can be observed in the motion picture eating their way through the medium. Even ganglion cells move. Single cells break off their contact with the others, especially before dividing, and they rejoin the others. Cells do not only adhere to each other by stickiness, but form protoplasmatic connections through which material, pigment, e.g. has been seen carried from one cell to the other. Stimuli also are transmitted from cell to cell since waves of ciliary movement run along a row of cells and can be interrupted by injuring one cell. [41] The growth curve of a single cell in tissue culture turned out to be a counterpart of that of a bacterium. Mutant strains of such cells "have been identified, isolated and established as standard stocks." Some mutant characteristics have remained stable after more than 200 generations, corresponding to stable mutations of microorganisms. These astounding similarities between the metazoan cell and the free living unicellular organism justify the designation of the body cell as a unit of life. Virchow and the men who shared his opinions could hardly know how right they were in many respects.

The dominance of morphological studies under the influence of Virchow and his school has misled people into identifying

the cellular doctrine with a one sided morphological attitude that neglects functional, physiological aspects. Virchow's numerous statements to the contrary have been quoted so often that I am reluctant to repeat them. May I just point to his one dictum that "pathological physiology is the main fortress of medicine while the pathological anatomy and the clinic are outlying bastions".[43] He postulated in his early years, and in 1900, that pathology should be pathological physiology. Young Virchow's first great scientific accomplishment was experimental, namely the demonstration of embolism (he coined the term).

But the fact remains that he and his pupils worked mainly in tissue pathology. People who complain about this and lay it at the door of cellular theory do not realize that this morphological work created the basis for later physiological studies. Furthermore, no methods were available at the time for cellular physiology, and physiology did not furnish ideas for cellular study. The anatomist Fleming,[44] who did classical work on cells and cell division, stated in 1882 that cell problems mostly belonged to the field of physiology, especially physiological chemistry. "But," he continued, "both seem hesitant to direct their action into a truly microscopic area." And as late as 1900 Eugen Albrecht, who was a man of vision, was pessimistic about a future cellular physiology. Modern techniques have made it possible.

The terms cytochemistry and biochemical cytology refute the idea that the cellular concept is necessarily a morphological one. These disciplines remedy the antiquated situation in which biochemistry did not apply the localization principle, be it Morgagni type according to organs or Virchow type according to cells, and they bring the cellular aspect once more into the foreground of medical science. Clinical blood chemistry, notwithstanding its practical usefulness, is like touring a state in a low flying airplane and counting the number of freight cars that are loaded with coal. That will not tell us how much coal is burning in furnaces, lying in covered sheds or still underground, and this may be more important than the amount in transit. Pflueger's proof that metabolism takes place in the tissues and not in the blood was the real death blow for humoral pathology. A recent paper, "The cellular principle in today's biochemistry,"[45] describes the chemical accomplishments of the cell in detail, how it transports glucose and aminoacids even against the concentration gradient, how it keeps sodium out of the cell, and how the membranes which surround the nucleus and the mitochondria function in similar ways. It concludes that modern biochemical knowledge shows the correctness of the cellular principle.

The functional completeness of the single cell decreases with evolution, it is best preserved in the sex cell. But many mammalian cells do have a specific metabolism which represents a functional separateness perhaps inherited from a chemical mutation in a single cell, and how shall one interpret the different phases of secretion in neighbouring glandular cells if not by individual differences in reactivity?

The division of matter from energy having disappeared, it has become meaningless to separate form from function. If we elaborate Karl Ernst von Baer's delightful idea of having our life extremely compressed in time or extremely stretched, and imagine an exaggerated slow motion picture of a living cell at a magnification that shows the molecules, then substance and function will be one, and histology will be identical with cytochemistry. A hundred years ago, Virchow said that the basis for the normal and pathological life of the cell was to be found for the time being in histology, in the finest anatomy. Morphology to him was the way to physiology, and pathology as well as biology have travelled this road a good distance in the last century. Thus it does not sound too paradoxical any more when we say that biochemistry is micro-micro-anatomy, that in the ever flowing stream of a living system, a histological aspect and a functional phase are identical, or that "the anatomy of an organism is the greatly magnified expression of its chemistry.[46]

Life processes, like everything else, take place in a space-time configuration.

It is an ever returning wish to make biology or one of its branches like pathology into an exact science. This was Virchow's dream, and he went a long way in the right direction. Almost 70 years after the publication of the "Lectures on Cellular Pathology," Ricker, this sharp opponent of Virchow, gave his book the title "Pathology as Natural Science," [11] and in 1950, V. Bertalanffy closed his paper on "Open Systems in Physics and Biology" by expressing the hope that this will "pave the way for biology to become an exact science." [47] Finally in 1955, the anatomist Zeiger wrote that it does not appear impossible anymore to resolve cytology into physics and chemistry.[35] The cellular doctrine opened the way to this desired goal.

Science often has been thwarted by ecclesiastical or political interference, and the cellular doctrine has not been spared. An Austrian book from the National-Socialist era lets the "crazy idea" of cellular pathology originate in the brain of a French Jacobine (meaning Raspail), and Virchow, it says, succeeded by a coup overnight in introducing it, because Germany, at the time, was infected with liberalistic, materialistic ideas.[48] Conversely we learn from a paper, which appeared in Warsaw in 1950, that cellular theory is built on idealism and reactionary metaphysics, and that its vestiges must be eradicated.[49] In the same year, a new "dialectic-materialistic" cell theory by Lepeshinskaya gained official recognition in Russia and was given a Stalin prize. This theory revived Schwann's belief that cells are formed in a fluid, a blastema, which belief, ironically, had represented idealism to the Nazi, in opposition to Virchow's materialism. Fortunately Lepeshinskaya has been disavowed lately,[50] and the "omnis cellula e cellula" is recognized again in Russia. Such things appear to us extreme, almost unpardonable; but how will future historians judge our generation's attitudes toward the Malthus problem or to the genetic danger of radioactive fallout?

Today's situation of medicine is not unlike that of 100 years ago in some respects: The improved microscope of 1830 yielded a multitude of not always easily digestible new facts, and the electron microscope does the same today. One-sided theories had to be and are to be dealt with: humoral and solid pathology then, systems like those of Ricker and Speransky in our time. And the danger of metaphysics encroaching upon medical science which was so great in the "Romantic" era should not be underrated today either. We still live in the era of cellular pathology and therefore are unable to judge it historically. Its beginnings, three generations back, are sufficiently removed, and we can say that the cellular doctrine has freed biology and medicine from metaphysical chaos and thus gained lasting fame. Future history of science will have to decide about the merits of anticellular theories because they keep us from a one-sided attitude and about their demerits because they raise anew the spectre of metaphysics. Philosophical minds have often dealt with analogies between world and man, considering both as entities, and the extremely holistic anticellular attitude evokes the saying of a 16th century humanist: "The world is one; not the sum total of atoms that are subject to chance, devoid of reason and incapable of building up an orderly cosmos." [51]

Our ego feeling, the consciousness of the human being as an indivisible unit, may be the real basis of the biological totality concept. But none of the theories which accuse the cellular doctrine of neglecting the organism as a whole has helped us in grasping the genesis of our ego consciousness.

As long as we have no method for attacking the problem how the totality of the metazoan organism is realized, it will remain preferable to study the parts that make this totality, the cells, the tissues, the organs. I am no judge of how far the concept of the whole organism as an open system in stream equilibrium (Bertalanffy) has heuristic value, with the mathematical

possibilities which are claimed for it. But it seems that the single cell with its individual metabolism also represents such a system.

People will react in different ways to a dictum like "every branch of science has its roots in metaphysics." [17] It may be easier to accept Goethe's formulation: "My way of looking at things is my thinking, and my way of thinking determines the way I am seeing." This shifts from metaphysics to psychology, and it catches the pivotal word "theory" in its original meaning of "to see." Even the sober Darwin once said that there is no good observation without a theory. The struggle between cellular doctrine and the totality aspect of living organisms is based to a large extent on differences of personality. The totality aspect is tempting esthetically; the whole, the all embracing, has more appeal than the details, one can feel like Faust compared with his unimaginative famulus. The philosophically trained Virchow knew how to deal with such feelings. After describing how the analytical method leaves us with the parts in our hands instead of the whole, he continues "is not this whole destructive (zersetzende) natural science a dead end street, and is it not truly high time that we turn back to other pathways? If only there were any: But we have no choice." [12, 51] The holistic aspect does not only satisfy philosophical urges, it has inestimable value in practical medicine. No good therapeutics are thinkable without it, the total person should always be treated not the localized disease. But psychotherapy also must have a means of entry, which may be represented by influencing a cerebral field of energy about whose function we have not even a nebulous idea. All this is beside the point when we look at theories as tools of science, as means of finding out more and more about the world around us and in us. The whole, from the empirical standpoint, always remains something we add by thinking to the parts we perceive, (ein Hinzugedachtes). Fr. Kraus,[52] who was the protagonist of constitutional medicine, saw no discrepancy between it and cellular doctrine. He believed that cells retain a certain autonomy within the organism they form. The question thus becomes quantitative. Do we think more of a federation (Staat) of rather independent cells as Virchow was inclined to do? Or do we think of mutually dependent cells which act together somehow through the blood and the nervous system. This "somehow" is the crux, the great question mark, of biology and natural philosophy. Schwann had recognized it clearly, twenty years before the Lectures on Cellular Pathology appeared. Virchow accepted the concept of the unity of the organism in the realm of psychology. His slogan for the practising physician: "to console, to alleviate, to cure" shows that the analytic cellular theory need not interfere with a healthy holistic attitude toward man, in medicine or otherwise. Few will deny that the body consists essentially of cells and their products since this is an observation not an interpretation. Neither Virchow nor his pupils have, to my knowledge, ever maintained that the cells lead an entitrely independent life, they only underestimated the importance of interrelations.

The importance of the cell as the smallest unit of life has been denied not only by those who prefer larger units, but also by those who in atomistic fashion look for ever smaller ones.

The many colorful names given to imaginary units have disappeared from the literature, and so have Haeckel's anuclear protists and the "naked" nuclei. Locke, in "ignorabimus" fashion, declared that man, even with instruments, could never hope to see the smallest units of biological function. R. Altmann in 1890 wrote a much discussed monograph with the title "The elementary organisms and their relationship to the cells." He described granules, "bioblasts", as the ultimate units of living matter. The cell appeared too complex for him for being the unit of life. I would rather say that life is extremely complex, and a simple unit therefore is unthinkable.

The idea even has been proposed that the cell can disintegrate into minute particles each of which will give rise to a new cell in analogy to sporulation. Electron micro-

scopy has brought many details of cellular structure to light, but not one of them so far resembles a free living organism. Nor do we see the postulated symbiotic conglomeration of virus sized living particles. Granting that a gene and a virus may sometimes be identical, and that viruses are organisms in a way, the fact remains that viruses do not lead an independent life but need the host cell. In this respect, the much compartmentalized cell still is the smallest unit of complete independent life, and there seems to be consensus about this. When people doubt the role of the cell as a fundamental unit because a denucleated fragment of an amoeba retains properties of living matter for some time, then the conclusion does not appear cogent. The anuclear fragment cannot divide and thus survive, it can ingest particles but cannot digest them, it can use glucose as a source of food but cannot synthesize nitrogenous compounds from glucose and urea. A denucleated plant cell does not form a new cellulose wall unless it is in contact with a complete cell.

Similarly, a denucleated egg cell can sometimes undergo cleavage, but development does not go further than the blastula, and the anuclear cells of such a blastula do not react to induction stimuli. The addition of a nucleus to the previously denucleated egg cell is followed by normal development in many instances. We do not know how far the evolution of life out of ordinary molecular structures took place slowly in minute steps or what role sudden, mutation like, changes have played. No matter how this has been there must have been precursors to the cell. But nothing compels us to assume that any existing forms resemble the postulated precursors. To take viruses as the precursors of cells is as unfounded as calling the chimpanzee the ancestor of man. We may discover smaller entities that possess some, but not all, characteritics of life, but we will have to be wary about assigning to them ancestorship to the world of cellular life. Maybe we should declare a moratorium on theories about the microstructure of living matter until a larger body of electron microscopic data and corresponding microchemical data is available. But I seriously doubt that we ever shall find a "unit of life" at the Angstrom level.

The concept of the atom as the smallest particle of matter existed for two millennia before methods were devised to prove its reality; that means: theory preceded observation. It was the converse with the cell since cellular theory came about 150 years after cells had been seen. Virchow, in 1855, considered it easier to build the basic ideas of biology upon the cellular elements than to build physics upon molecules and atoms since "cells are within the boundaries of sensual perception while the units of the physicists, the molecules and atoms, are only conclusions from sensual perceptions and are philosophically so little satisfactory that we can look at their assumption only as a temporary termination of research." [13] The inner structure of cells as well as atoms appears more and more complex with improving methods of investigation. Both were originally considered static structures but appear now as dynamic systems.

A historical comparison between physics and biology as far as small units are concerned may not be out of place. How real is the connection between the atomic ideas of Democritus and the modern concept of atoms, and what about the relationship between cellular theory and Leibniz's monads? Dalton's publications on atoms, which began in 1803, were in part responsible for the interest in small units of living matter. But the urge to know about the atoms of life had been strong for generations in the best minds: Leibniz, Locke, Buffon, to mention only a few. The divisibility of matter, living or not, appears as a logical postulate, a thought that arises independent of observation. Thus when Virchow applied the recently proved cellular composition of the animal body to the often voiced idea of everything consisting of small units he fulfilled a strong logical desire of his generation, and similar ideas had been voiced in the decades before him by people who had not made original observations. The large number of new

anatomical observations that had accumulated since 1830 made a theory into which new facts could be fitted doubly welcome. The comparison between physics and biology may have entered a new phase in our days with the concept of feedback. How far do we think of the computer as a whole, a totality, and how far do we think of the single tubes or transistors. One might call that a "seeming problem" (Scheinproblem) not a true problem but could one not say the same of the totality aspect and the cellular aspect in biology?

The cells serve two masters, one rules from inside the cell by Virchowian law, the other resides in the organism of which the cells are a part, but where and how he rules is unknown. In the fertilized egg, the cell and the organism are the same. When Spemann, using a fine hair, carefully separated the two blastomeres which had resulted from the first division of an amphibian egg, each cell developed into a complete larva, in spite of the fact that it was not under the influence of the total organism. When Roux, the founder of experimental embryology, had attempted in 1888, the same experiment by destroying the one cell with a hot needle, the result had been different: only half an embryo was formed. The remaining cell was prevented from following its inherent power of forming a complete larva by the remnants of the destroyed cell which so to say reminded it of its dependent status in relation to the total organism. Removal of certain cells from a young embryo generally leads to malformation of the larva because the total organism has been disturbed. But careful removal (Ubisch) has, in certain animals, resulted in defects of only that part of the body that corresponded to the removed cell.[54] That means: the totality influence was nil, only the removed cell counted. This ambivalence is demonstrated in grotesque fashion by the chimaera experiment. A fragment of a young frog embryo is taken from the area that is destined to become central nervous system, and is transplanted into that part of a salamander embryo that will form the mouth. The fragment will develop into a mouth, in spite of the fact that it was meant to become nervous tissue; thus it obeys the law of the total organism to which it now belongs. But this mouth will be a frog's mouth in a salamander body because the transplanted cells also obey their inherent nature.

Thus, there is no yes-or-no-answer to the question of independence or dependence of the cell. Simple answers hardly ever do justice to the complexity of life processes.[55] Even the staunch enemy of cellular doctrine, M. Heidenhain,[56] considered the antithesis between cellular dominance and organismic dominance as erroneous since "all and everything in the living body is based on interrelation between the whole and the part." But who, may I ask, no matter how philosophy minded he may be, can refrain from searching for the source and seat of this mysterious force which the whole exerts over the parts? We come back to the above quoted word of the 80 year old Goethe: "Thus one is driven from the whole to the detail and from the detail to the whole whether one wants to or not." We can expect the cell to remain, for some time to come, if not our unit of life so certainly our main unit of investigation, and the cellular doctrine will remain a most valuable guide in the progress of biology and that part of it which we call medical science.

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The Insecticide Society of Washington

C. M. Smith

In January 1959 there was affiliated with the Washington Academy of Sciences an organization known as the Insecticide Society of Washington. It is believed that members of the Academy will be interested in an account of the founding and nature of this organization, and this seems an appropriate time to so acquaint them because the Society was started by Dr. F. L. Campbell, the 1959 President of the Academy, and October of that year marked the 25th anniversary of its founding.

The U. S. Department of Agriculture has long been interested in avoiding or controlling the damage caused by insects attacking crops, farm animals, and even man himself. As the interest in controlling these pests grew it was natural that chemical compounds were tried and recommended for the purpose. The manufacture and sale of such chemical insecticides assumed such dimensions that there arose a need for some form of control over their purity and for keeping a check-rein upon the validity of the claims made for their efficacy. In 1910 the U. S. Congress passed the so-called Insecticide Act. The Insecticide and Fungicide Board, composed of representatives from the U. S. Bureaus of Entomology, Plant Industry, Animal Industry, and Chemistry, was appointed to administer the act. There also was established in the Bureau of Chemistry an Insecticide Division for the analysis of chemical insecticides. Naturally, need arose for a considerable amount of research work to develop the knowledge necessary properly to enforce the act. The regulatory and research activities were finally separated, with each group building up laboratories staffed by scientists of various kinds.

In October 1934, Dr. F. L. Campbell, an entomologist in the Division of Control Investigations of the Bureau of Entomology, recognized the need for closer personal association of the scientists in the Washington area who were working in the field of insecticides, particularly to provide for informal exchange of ideas and discussion of technical and scientific problems. In a letter dated October 4, he reported to the Chief of his Division the results of a meeting held at his house the previous evening. This meeting was attended by all members of his own laboratory and by Dr. R. C. Roark and Mr. C. M. Smith of the Insecticide Division, Mr. W. S. Abbott of the Insecticide Testing Laboratory at Beltsville, and Dr. C. A. Weigel from the Truck Crop and Garden Insects Division. This group decided to hold an organizing meeting on October 17 and this action was then called to the attention of all those men in the nearby laboratories of the Department of Agriculture and the University of Maryland at College Park, who conceivably might be interested. This meeting was held as planned in Dr. Campbell's laboratory at 7710 Blair Road, Takoma Park.

The response was very gratifying, 55 persons attended. A temporary committee composed of Mr. W. S. Abbott, Chairman, and Drs. L. A. Hawkins, R. C. Roark, E. H. Siegler, and C. A. Weigel, was appointed to suggest an outline for the nature of the organization and the rules and regulations to govern the conduct of future meetings. This committee met in the Takoma Park laboratory on October 24, 1934 and drafted such a set of recommendations. They then set the date for the next meeting November 14, at which time temporary chairman, Mr. W. S. Abbott brought the recommendations before the membership and the following rules were adopted in lieu of a formal constitution and by laws:

1. The name of the organization shall be "The Insecticide Society of Washington".

2. The membership shall be limited to federal and state men who are interested in insecticides.
3. A nominating committee shall be appointed to suggest candidates for the following offices: Chairman, Vice-Chairman, Secretary, Treasurer, and a "Steering Committee" to be composed of the first two officers and three other members.
4. The Steering Committee shall be authorized to perform all the functions that are usually assigned to several committees in larger organizations; i.e., executive, program, membership, arrangements for meetings, and refreshment. This committee shall endeavor to secure speakers best qualified to talk on the topic chosen. Non-members, including employees of commercial firms may be invited to speak.
5. Meetings shall be held preferably on the third Wednesday of each month from September to June, inclusive.
6. Dues shall be $1.00 per year per member.

The nominating committee suggested individuals for the four offices and the members confirmed Dr. F. L. Campbell as Chairman, Mr. C. M. Smith as Vice-Chairman, Mr. J. W. Bulger as Secretary, and Mr. S. C. Billings as Treasurer. Drs. E. L. Griffin, C. A. Weigel, and E. H. Siegler were chosen to be the steering committee. Following a *lapsus linguae* by the secretary, this committee came to be called for quite some time, the "steerage committee".

Some of these regulations were changed as time passed. The two offices of secretary and treasurer were merged into one and the election date changed to May, the officers then serving from July to June. At the suggestion of the Entomological Society of Washington, the Insecticide Society soon devoted the June meeting to participating in the annual picnic of that other affiliate of the Washington Academy. The most significant change, however, was to broaden the membership to include interested scientists and technical people from industry.

Since the membership of the Insecticide Society consisted mainly of entomologists and chemists, it immediately became the practice to elect a Chairman from among the former one year, and from the latter the next year. In most cases this convention has been followed to the present day.

The Society has used a number of meeting places over the years, as necessity and expediency demanded. The first ten meetings were held at Dr. Campbell's laboratory in Takoma Park. Meetings numbered 11 to 52, inclusive, were held at the nearby Jesup-Blair House on Georgia Avenue, just outside the District of Columbia. With the coming of World War II, this building was taken over by the county draft board and the next three meetings had to be held in other county buildings in Silver Spring. One of these was in a room over the police station and the other two over the liquor dispensary. The record does not make clear whether the latter two were better attended or more lively than those held elsewhere! Meetings numbered 56 to 62 were held at the Bethesda Recreation Center Building at 4700 Norwood Place, Bethesda. Finally, Dr. E. N. Cory, then Professor of Entomology at the University of Maryland in College Park, and State Entomologist of Maryland, kindly made arrangements for the Society to meet in his quarters in Morrill Hall at the University. This proved to be quite satisfactory and the Society met here until October 1954. That year the Entomology Department moved to new quarters in Symons Hall and the Society went along, meeting in lecture rooms and later in the auditorium.

One unique feature of the Society that deserves comment is that the dues of one dollar per year, established in 1934, have never been increased. Where else will a dollar buy as much today as 25 years ago?

In the course of 25 years over 300 papers have been presented at the meetings. The first two were given by Mr. J. S. Yip, a specialist in the cultivation of the insecticidal plant, pyrethrum, and Dr. F. B. Laforge, a chemist long identified as an authority on the chemistry of the insecticide

dally active constituents of this plant. It seems quite appropriate that the Society began its existence by consideration of this, one of the most important and interesting of plant insecticide materials.

Through the years the topics discussed have ranged over the entire broad field of pesticides and their use. There also have been excellent papers on such diverse subjects as plant growth regulators, photoperiodism in plants and insects, insect physiology and even scientific travelogues. The list of speakers includes bureau and division chiefs from various civil agencies, professors, representatives of the Armed Services, prominent authorities from industry and members of Congress. We also have had prominent guests from other countries from time to time. Among the early speakers were Mr. R. H. LePelley, a government entomologist from Kenya Colony, British East Africa, and Dr. F. Tattersfield, Head of the Department of Insecticides and Fungicides at the Rothamstead Experiment Station in England. The Society honored the latter at a dinner meeting at the Cosmos Club on May 31, 1935.

Now for a few words concerning the attendance at the meetings of the Society through the years. Naturally the number soon dropped below the 55 who attended the organizational meeting, but an average of 45 was maintained through the eight meetings of the first year. In April 1939, Dr. L. P. Ditman, then Secretary-Treasurer, presented a report on the attendance figures. He stated "Many individuals seemed rather surprised that the Society has existed for so long a period. Many are still rather uncertain as to the future of the Society" and concluded his memorandum with his own comment, "As far as the future of the Society is concerned, the Secretary feels that it will exist as long as a minimum of 20 good souls will continue to brave such nights as were experienced during January, February, and March of 1939". His faith in the Society was well founded, for although the attendance figure once dropped to 13, the Society has now lasted an additional 20 years. It also speaks well for the insight of Dr. Campbell and the other founders of the Society, that the purpose of the organization is still the same—a place where these "20 or more good souls" can meet in an informal atmosphere to enhance their knowledge of insecticides and like matters; to listen, to discuss, even to argue at times; and, not the least, to enjoy the company of our colleagues. It also is gratifying that the Society has the stature to be honored as an affiliate of the Washington Academy of Sciences.

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Science in Washington

SCIENTISTS IN THE NEWS

This column will present brief items concerning the activities of members of the Academy. Such items may include notices of talks given, important conferences or visits, promotions, awards, election to membership or office in scientific and technical societies, appointment to technical committees, civic activities, and marriages, births, and other family news. Formal contributors are being assigned for the systematic collection of news at institutions employing considerable numbers of Academy members (see list on masthead). However, for the bulk of the membership, we must rely on individuals to send us news concerning themselves and their friends. Contributions may be addressed to **S. B. Detwiler, Jr.,** Associate Editor, 2605 S. 8th St., Arlington, Va.

Applied Physics Laboratory

Alfred J. Zmuda has been appointed a consultant to the Geophysics Panel of the Air Force Scientific Advisory Board.

Coast & Geodetic Survey

Dean S. Carder was appointed chief seismologist of C&GS in October, 1959. During December he was elected a member of the Scientific Council of the Geological Society of Washington; also, he participated as observer and analyst in Phase I of the Louisiana "Cowboy Shots." Dr. Carder is serving as president of Rapidan Camps, Inc., a cooperative camping group in the Blue Ridge.

Charles A. Whitten, chief of the Triangulation Branch, presented a paper on the computation and adjustment of geodetic networks as part of an international symposium held at Cracow, Poland, September 9-13, 1959. The presentation was made by invitation of the Polish Academy of Sciences.

Aaron L. Shalowitz has been elected a fellow of the American Geographical Society. Dr. Schalowitz represented C&GS at the recent Third Annual Meeting of the Florida Shore and Beach Preservation Association.

David G. Knapp recently visited Peru, Chile, Argentina, Brazil, and Puerto Rico, to confer with geophysical scientists; he was accompanied by Mrs. Knapp. Dr. Knapp participated in the Symposio Antartico de Buenos Aires, held November 17-25 under the sponsorship of the Instituto Antartico Argentino.

Thomas J. Hickley was recently made chief of the Instrument Davision.

Geological Survey

At the annual meeting of the Mineralogical Society of America in early November, **George Switzer** was elected secretary, and **Marjorie Hooker** the treasurer of the organization. **George T. Faust** was appointed to the Nomenclature Committee for 1960-62, while **Edwin W. Roedder** was appointed to the Board of Associate Editors for 1960-62, and to the Awards Committee.

At the annual meeting of the Geochemical Society last November, **George T. Faust** was re-elected treasurer and reappointed to the Advisory Board on the International Critical Tables.

Francis R. Fosberg attended AAAS Council meeting in Chicago, December 26-30. While in Chicago he presented a paper, "Problems of Tropical Herbaria," before the Society of Plant Taxonomists on December 28. On December 10, Dr. Fosberg sat with the NAS Committee on Science in UNESCO, as U. S. member on the Advisory Committee on Science Research in the Humid Tropics.

While on a vacation trip in November and December, **Margaret D. Foster** visited points of interest in Morocco, Spain, Majorca, Sicily, Italy, and southern France.

National Bureau of Standards

On January 4, Director **Allen V. Astin** resumed his duties on a part-time basis following recuperation from a heart attack.

Three NBS staff members presented invited papers at a symposium on weights and measures at the Chicago AAAS meetings, December 28-29. **Lewis V. Judson** spoke on "Our Units of Weights and Measures"; **Irvine C. Gardner** discussed "Adoption of a New System of Weights and Measures"; and **Wilmer Souder** presented "Metric Usage—Report of Special AAAS Committee." At the same meeting, **George C. Paffenbarger** spoke on "Dental Materials from 1939 to 1959." Dr. Paffenbarger is a research associate of the American Dental Association, stationed at NBS.

Two staff members have been appointed to editoral boards of the American Chemical Society for the three-year term 1960-63. **Roger G. Bates,** assistant chief of the Chemistry Division, was appointed to the Advisory Board of *Chemical and Engineering News,* and **Bourdon F. Scribner,** chief of the Spectrochemistry Section, will serve on the Advisory Board of *Analytical Chemistry.*

Kurt E. Shuler spoke on "Energy Transfer and Relaxation Processes in Gas Phase Collisions" at the Harvard MIT Physical Chemistry Colloqium held at Cambridge on January 7.

Walter J. Hamer, chief of the Electrochemistry Section, on December 15 was elected vice president of the Electrochemical Society for the three-year term 1960-63. He will assume office on May 1, at the Society's national meeting in Chicago. On completion of his term as vice president, Dr. Hamer will become president of the Society for the 1963-64 term.

On January 5, **William J. Youden** presented a paper on "Experiments Involving Several Variables" at the Air Reduction Corporation, Murray Hill, N. J., and a paper on "Statistical Ideas Useful in Experimentation" at a meeting of the Society for Applied Spectroscopy in New York City.

William F. Meggers, former chief of the Spectroscopy Section and now retired, left Washington January 15 on a trip around the world, accompanied by Mrs. Meggers. The couple plans to visit Near- and Far-Eastern countries during a 60-day tour.

Alan D. Franklin was appointed acting chief of the Mineral Products Division in November; he replaces **Irl C. Schoonover,** who will devote full time to his duties as associate director for planning. Dr. Franklin, who received the Ph.D. degree from Princeton in 1950, served as chief of the Franklin Institute's Magnetics Section from 1945 to 1955, when he came to NBS. Since joining the Mineral Products Division he has conducted research on the fundamental properties of ferroelectric materials.

Karl G. Kessler was appointed chief of the Spectroscopy Section last August. In this capacity Dr. Kessler will direct work in interferometry, analysis of spectra, and the study of hyperfine structure and isotope shift in spectra; additionally, he will continue to head the Bureau project to develop a new wavelength standard of length, using atomic beams as a light source.

National Institutes of Health

Erich Mosettig of the Laboratory of Chemistry, National Institute of Arthritis and Metabolic Diseases, returned to Bethesda December 23 after an unusual international lecture tour of extensive propportions. The trip, sponsored jointly by NIAMD and the Cancer Chemotherapy National Service Center, involved over 25 formal lectures and informal talks before scientific groups in Honolulu, Japan, Hong Kong, Thailand, India, Pakistan, Lebanon, Turkey, and Austria. These lectures covered various aspects of the NIH research program, including the biochemistry of steroidal compounds, modern analgesics, endogenous carcinogenesis, and cancer chemotherapy.

State Department

Wallace R. Brode, science adviser to the Secretary of State, has won the American Chemical Society's 1960 Priestley Medal, according to recent announcement by retiring ACS President John C. Bailar,

Jr. The gold medal—highest honor in American chemistry—was awarded to Dr. Brode for "distinguished services to chemistry"; it will be presented at the 137th national meeting of ACS in Cleveland next April. Immediate past president of AAAS, Dr. Brode is on leave from the National Bureau of Standards, where he has been associate director for the past 12 years; he was for 20 years professor of chemistry at Ohio State University.

Deaths

Rees F. Tener, assistant chief of the Testing and Specifications Section, National Bureau of Standards, died suddenly on December 25. A native of Sinking Springs, Ohio, Mr. Tener received the M.S. degree from GWU in 1926. After Army service during World War I and science teaching experience, he joined the NBS staff in 1924, serving in the Rubber Section until 1930 and thereafter in the Testing and Specifications Section. Mr. Tener, a recipient of the Commerce Department's Meritorious Service Award, was an authority in various fields of rubber chemistry and on the development of specifications and test methods for organic and fibrous products. He was particularly active in committee work for ASTM and the American Standards Association.

Walter J. Murphy, editorial director of the American Chemical Society's applied journals, died of cancer on November 26. Dr. Murphy's death came at the height of a notable career devoted to bettering the chemical profession and industry. Under his leadership over a 17-year period, the ACS applied publications became the largest scientific publication program in the world, with a total circulation of over 165,000. He was also director of the Society's News Service, the public relations organization serving the chemical profession.

Following a successful start in the chemical process industries, Dr. Murphy entered the technical publication field in 1930, when he was named managing editor, and later editor and general manager, of *Chemical Industries.* Subsequently he was named editor of *Chemical and Engineering News* and *Industrial and Engineering Chemistry.* In 1953 he initiated the *Journal of Agricultural and Food Chemistry,* and in 1959 the *Journal of Chemical and Engineering Data.* In 1947 he had received the honorary D.Sc. degree from Centre College of Kentucky.

AFFILIATED SOCIETIES

As time goes on, we hope to establish in this section of the Journal periodic reports of the activities of the some twenty-six scientific bodies affiliated with the Washington Academy of Sciences, reports which will be of interest and service to members of the Academy. Experience will indicate, as we go along, just what sorts of material are both obtainable and worthwhile, but at this stage we have in mind such items as: meeting places, dates, and topics, special projects, summaries of society proceedings, educational and promotional programs, public relations efforts, and so on. Machinery is gradually being set up whereby one individual in each affiliate will keep us informed of what goes on in his particular organization. In addition, we will include, as data become available to us, names of incumbent president and secretary of each society listed, with indications of changes as they occur.

We solicit information from individual members of the Academy as a most welcome supplement to the established channels.—**Russell B. Stevens,** Associate Editor, George Washington University.

American Meteorological Society, District of Columbia Branch

Oct. 21, panel discussion on the "Use of Artificial Satellites in Meteorology," with Sigmund Fritz, David Wark, Charles Bristor, Jay Winston, Lester Hubert, and David Johnson, all of the U. S. Weather Bureau, participating.

Nov. 18, address by A. H. Mikesell, U. S. Naval Observatory, on "Finding the Jet Stream by Star Twinkling."

Dec. 16, panel discussion on "Oceanographic Forecasting," with John Schule, Richard James, Howard French, and Walter Wittman, all of U. S. Navy Hydrographic Office as participants.

Jan. 20, meeting in Lecture Room, NAS-NRC, 2101 Constitution Ave., N.W., 8:00 P.M., with a talk by Albert P. Crary, Geophysical Research Directorate, on "Antarctic Meteorology."

Feb. 10, meeting scheduled as above, to hear Joseph Smagorinsky, U. S. Weather Bureau, discuss "Digital Simulation of the General Circulation."

American Society for Metals, Washington Chapter

President: William L. Holshouser (NBS). *Secretary:* Glenn W. Geil (NBS)

Nov. 16, meeting and address by Robert F. Thompson, General Motors Corporation, on "Automotive Gas Turbines."

Dec. 21, joint meeting with AWS and lecture by Alexander Lesnewich, Air Reduction Company, on "Electron Beam Welding."

Jan. 18, Meeting and talk by Morris Tanenbaum, Bell Telephone Laboratories, on "Metallurgy in Electronics."

Feb. 8, Burgess Memorial Lecture, Arlington Towers, by R. H. Aborn, U. S. Steel Corporation.

March 21, John E. Hilliard, General Electric Company, "Pressure - Induced Transformation in Metals."

April 18, Walter L. Finlay, Crucible Steel Company, "Titanium and Competitive Stainless Steels."

May 16, National Officers Night, featuring address by the National President, Walter Crafts, on "Facing the Productivity Challenge: Men and Metals of the Next Decade."

The Washington Chapter, ASM, sponsored a series of six Thursday Night Talks on Metallurgy, Oct. 29-Dec. 10, for high school students, metal workers, and others. A second series, sponsored by the NBS and local technical societies, is scheduled for the Department of Commerce auditorium at 7:30 P.M., on Tuesday evenings, Jan. 12, 19, 26 and Feb. 2, 9, on the general topic: "Materials Development in the Space and Atomic Age." All interested persons are invited.

Anthropological Society of Washington

Three meetings were held in the fall of 1959, as follows: (1) Oct. 27, Carlson Gadjusek, National Institutes of Health, an illustrated lecture on "Child Growth and Development Patterns in New Guinea"; (2) Nov. 16, Eugene I. Knez, The Smithsonian Institution, a "Korean Dance Festival," featuring dances in costume by five Korean women and children; and (3) Dec. 11, Louis J. Luzbetak, two color films on tribal life in the interior of New Guinea.

Botanical Society of Washington

President: Harold T. Cook (USDA). *Corresponding Secretary:* Muriel J. O'Brien (USDA)

Dec. 1, presidential address by H. C. Hanson, Catholic University, on the flora of Alaska.

Jan. 6, regular meeting and presentation by H. C. Murphy, Department of Agriculture on "Oat Diseases are Shifty Enemies."

Feb. 2, next regular meeting, Powell Auditorium, 2170 Florida Ave., N.W., at 8:00 P.M.

Chemical Society of Washington

President: Allen L. Alexander (NRL). *Secretary:* John L. Torgesen (NBS).

Dec. 10, general meeting of the society, featuring an address by Wallace R. Brode, Department of State, on "Formulation of a Science Program." A Board of Managers meeting, on the same date, under the chairmanship of President W. W. Walton, heard reports as follows: (1) Committee on Education—CSW is cooperating with the ACS Division of Chemical Education in canvassing local high schools for requests for the Visiting Lecturer Program; (2) Committee on Programs—arrangements are being completed for a joint meeting with the Maryland Section of the ACS, probably Friday, May 6, at the University of

Maryland; (3) from R. F. Gould of ACS Headquarters concerning the chemistry merit badge of the Boy Scouts of America —which problem will be studied by the Committee on Education.

Geological Society of Washington

President: Harry S. Ladd (USGS). *Corresponding Secretary:* J. Thomas Dutro (USGS).

Dec. 9, address by retiring President Joseph W. Greig, Carnegie Institution, on "Development of Phase Equilibrium Studies in the Interest of Petrology."

Jan. 13, council meeting.

Helminthological Society of Washington

President: George W. Luttermoser (NIH). *Corresponding Secretary:* Edna M. Buhrer (USDA).

Dec. 1, joint meeting with the Washington Tropical Medicine Association and the Howard University Chapter of Sigma Xi, at Howard University, at which Sir Phillip Manson-Bahr, London School of Tropical Medicine, spoke on "Wild Game and Man in Central Africa," illustrating with slides the life-cycles of some common parasites of man and animals.

Jan. 20, meeting at 8:00 P.M., McMahon Hall, Catholic University, with two scheduled addresses: (1) Benjamin Schwartz, "Discovery of Trichinae and Determination of their Life History and Pathogenicity"; and (2) Major R. I. Anderson, "Serological Diagnosis of *Schistosoma mansoni* Infection."

Feb. 17, meeting at University of Maryland.

Mar. 16, meeting at National Institutes of Health.

Oct. 8, Fiftieth Anniversary meeting of the Society, with morning and afternoon scientific programs and an evening banquet.

Society for Experimental Biology and Medicine, District of Columbia Section

President: George A. Hottle (NIH). *Secretary:* Edwin P. Laug (FDA).

Dec. 3, four papers as follows: (1) H. Wishinsky, E. Poole, and S. P. Erkel, Sinai Hospital, Baltimore, "Gamma Globulin Separation Using 'Rivanol'"; (2) Jiro Oyama and R. Lorimer Grant, FDA, "Serum Insulin-like Activity as Measured by the Mouse Diaphragm Method"; (3) Elsworth R. Buskirk, NIH, "Study of Human Energy Metabolism as Influenced by Food and by Cold"; and (4) Emilio Weiss and Harry R. Dressler, NMRI, "Centrifugation of Rickettsiae and Viruses onto Entodermal Cells and its Effect on their Absorption."

Feb. 4, 8:00 P.M., program of scientific papers in Hall A, The George Washington University School of Medicine, 1335 H Street, N.W.

April 7, meeting at same location.

June 2, Annual Dinner Meeting, time and place to be announced later.

ACADEMY ACTIVITIES

Board of Managers. January Meeting

These notes are intended to outline briefly, for the information of the membership, the principal actions taken at Board meetings. They are not the official Minutes as prepared by the Secretary.—Ed.

The Board of Managers held its 523rd meeting on December 15 at NAS, with President Campbell presiding.

The Secretary (Dr. Specht) distributed draft Minutes of the 521st and 522nd meetings of the Board. Approval was deferred until the next meeting.

Dr. Campbell reported that in recent balloting on the proposal to increase Academy dues from $6 to $10 annually, only 61 percent of the voting members had favored the change. Since a two-thirds majority was required for adoption, the proposal had failed.

Dr. Campbell reported on the Executive Committee meeting held just prior to the Board meeting, and on a meeting held December 14 between Academy officers and a number of members from the Smithsonian Institution. At the latter meeting,

the Smithsonian members had expressed concern over recent Board actions directed toward changes in the *Journal*—that is, from a publication containing chiefly scientific material of a descriptive character, as in the past, to a publication containing original cross-disciplinary articles as well as news of the Academy and local scientific activities. The Smithsonian members had felt that they should have been given an opportunity to vote on the proposed changes.

The Board agreed that it had acted within its authorization in approving a curtailment of the *Journal*—and indeed, that such curtailment was imperative in view of the failure of the dues increase proposal. However, it was decided that the Secretary should conduct a referendum to determine the preferences of the membership as to the content of the magazine.

Dr. Goldberg reported on two informational resolutions passed at a recent meeting of the Philosophical Society. The Society felt that the Academy should reduce its capital before increasing dues. Secondly, the Society indicated its expectation to publish its papers in national journals, its Minutes in the *Journal* of the Academy.

Chairman Biberstein of the Committee on Awards presented the Committee's recommendations on award recipients at the next annual meeting of the Academy, in the categories of physical sciences, mathematics, engineering sciences, biological sciences, and teaching of science. His report was accepted with the Board's commendation for its thoroughness.

Chairman Schubert of the Committee on Encouragement of Science Talent reported on current activities of the Washington Junior Academy of Sciences. He mentioned that the Junior Academy had scheduled a meeting on December 28 for the presentation of scientific papers, and that at least a third of the membership was expected to be present.

Dr. Schubert introduced David Chen, president of the Junior Academy. Mr. Chen presented a proposed change in Article IX of the WJAS constitution, which would change the procedures for amending the bylaws in order that 7th- and 8th-grade students might be admitted to the Junior Academy. The Board approved the proposed change.

Dr. Campbell presented the recommendation of the Executive Committee, that $4,000 be budgeted to the *Journal* for its expenses in calendar 1960. (This represents less than half the cost of the magazine in 1959, which is estimated at $9,500.) He estimated other Society obligations in 1960 at $8,475 and its income at $9,100, and that a deficit of about $3,400 could accordingly be expected. The Board approved the budget for the *Journal.*

Dr. Frenkiel objected to the apparent inconsistency of approving a budget for the *Journal* while simultaneously preparing to ask the membership's opinion on the future policy of the magazine. Dr. Campbell replied that the proposed referendum was concerned only with the content of the *Journal,* and that a reduction in size and frequency of issue, and hence in overall cost, had already been established.

Dr. Campbell presented a report on four Academy projects for which a 1960 proposal would be submitted to the National Science Foundation. These projects are a continuation of those undertaken in 1959, except that two are to be expanded; the total amount involved is $47,000. The Board approved submission of the grant request.

Chairman Kushner of the Membership Committee presented for first reading the names of seven candidates for membership in the Academy. Dr. Kushner thereupon presented for second reading the names of seven other candidates previously proposed, as follows: Roy J. Barker, Lafe R. Edmunds, Robert B. Fox, Stanley A. Hall, Thomas C. Hoering, Ronald E. Kagarise, and Gunnar Kullerud. These candidates were then elected to membership.

Dr. Campbell presented a report from Chairman Van Evera of the Committee on Grants-in-aid for Research, recommending

grants to two area high school students in a total amount of $141.82. The Board approved the grants.

In the absence of Chairman Shepard of the Committee on Bylaws, Dr. Gurney presented a third draft of the proposed revision of the Bylaws. It was agreed that this draft should be distributed to the Academy membership for consideration and approval.

The Amateur and the Academy

Summary of the Retiring President's Address, to be presented at the February 18 meeting of the Academy.

Because membership in the Washington Academy of Sciences has been restricted to those "who by reason of original research or scientific attainment are deemed eligible . . .," it has been and is an organization composed almost exclusively of professional scientists; that is, of those who earn their living in some kind of scientific work and who are usually highly skilled specialists. Although amateurs are not specifically excluded, few have been admitted to the Academy because as a rule they neither publish original research nor hold important positions in the scientific community.

Past-President Campbell will argue that amateurs, teachers of science, professional neophytes, and intelligent citizens who recognize the philosophic, economic, and social importance of science and seek to understand it should all be encouraged to apply for membership in the Academy after its Bylaws on membership have been suitably amended. He believes that amateur interest in science should be encouraged by the Academy, not only among young students, as is now being done, but among adults; that observation of nature offers the readiest and most interesting introduction to science for the amateur; and that the amateur can, if he tries, add something to the great body of informational data from which knowledge is derived. By way of illustration of what can be done, Dr. Campbell will show some beautiful color photographs of animal and plant specimens that were taken by two enthusiastic amateurs, both over seventy years of age. Frank L. Campbell

JUNIOR ACADEMY NEWS

By David Malin, Chairman
Publications Committee, W.J.A.S.

The December 28th convention of the Washington Junior Academy of Sciences held at the Burlington Hotel was the high point of the organization's activities in 1959. Thirty-three papers based on original research were given by junior scientists of this area to more than two hundred of their scientifically minded contemporaries. Separate sessions were held in Biology, Physics, Chemistry, and Mathematics.

To give some indication of the interest and achievement of this area's young scientists, the titles of the papers are listed here:

Biology

Iris Lipkowitz, "Observations on the Nutritional Requirements of *Spirostemum ambiguum*"

Grover Sherlin, "Information from Tree Rings"

Barbara Miller, "The Production of Antibiotics—From Mold to Medicine"

Morgan Morrison, "Determination of the Efficiency of Organic Pesticides on Common Economic Insect Pests"

Mark Levy, "The Sodium Chloride Tolerance of Certain Chlorella"

Carol Anne Love, "The Effect of Temporary Cooling on the Development of the Chick Embryo"

Betty Jane Sherlin, "Studies of Trees in Winter"

Heijia Lee, "Effect of Cholesterol on the Growth of Chorioallantoic Membranes"

David Chen, "Experiments on Hormonal Control of Insect Metamorphosis"

Dennis Marienfeld, "Do the Widely Advertised 'Germ Killers' Really Kill Germs?"

Harlan Himel, "The Cellular Slime Molds"

Daniel Wheeler, "Simple Techniques of Photomicrography Applicable to Biological Research"

Physics

Robert Schooley, "Radioactivity in Washington, D.C. Milk"

Janet Price, "What is a Cloud Chamber?"

Eugene Wengert, "Radioactive Probe Currents and Potential Gradient Related to Storms and Fair Weather"

Barbara Levin, "Analysis of Properties of Concrete"

David Malin, "A System for Automatic Optical Recognition of Printed Characters"

Dennis Herrin, "Solar Energy"

Jerome Dufour, "X-Ray Diffraction"

Michael Brownstein, "A Seismic Model Study"

Delo Mook, "Using a Tape Recorder as a Multi-Channel Recording System for Industrial Control"

Chemistry

Harold Weiler, "Paper Chromotography in Medical Research"

Barbara Blount, "Corrosion of Metals"

Raymond Baggs, "Correlation of Respiratory Decline with Amount of Vitamin E in Rat Liver Tissue"

Michael Mitchell, Jr., "Development of an Experimental Universal Wax Crayon from Ordinary Household Ingredients"

Margaret Ferguson, "Acid Production in the Mouth"

Cathy Briggs, "A Chemical Study of Carotenoids and Vitamin A in Invertebrates"

Gil Fritz, "Physical and Chemical Properties of Polymers"

Morgan Morrison, "Experimental Studies of Proteolytic Enzymatic Hydrolytic Systems"

Mathematics

Joel Dressler, "Universal Mathematics"

David Zalkind, "Theory of Probability and Pascal's Triangle"

Kenneth Taylor, "A Logical Approach to Space"

James Baker, "Game Mathematics"

JOINT BOARD

The Joint Board on Science Education of the Greater Washington Area has announced that it has planned expenditures for 1960 in the amount of $10,500. The items contained in this budget are:

Science fairs expenses-local, national	$ 3,100
Science teacher awards	750
Publication of The Reporter	2,200
Frontiers of Science lecture series	250
Research training projects	300
Publication of Science Projects Guide Book	2,500
Administrative, Committee, and Directory	1,100
Miscellaneous	300
Total	$10,500

In addition, the Joint Board has been intrusted with the administration of the 4-fold science projects of the Washington Academy of Sciences financed by a grant from the National Science Foundation. Although this grant amounts to $35,200, none of these funds may be allocated to the regular program of the Joint Board.

In consideration of other sources of income, the Board will need to raise $8,000 of the above $10,500. These funds are being solicited from technical societies and industrial and business organizations. Although all voluntary gifts from persons are acceptable, no solicitation of individual scientists and engineers is being made, in the belief that they participate indirectly in society or organization giving. Certainly their principal contribution is one of contact, counselling, speaking, or advising, within the Joint Board program.

The success of the local educational program lies in the unselfish services of many scientists, engineers, and teachers freely given and with extreme dedication. It would be very foolish to assume that the importance of this work has lessened. Only on ostrich-like individual could fail to heed to the warnings of Iron-Curtain countries concerning their plans to surpass us, particlarly in science.

Many technical societies of this area have been outstanding in their financial support of the educational activities of the

Joint Board. Others have given only token support. Every scientist in the Washington area is urged to encourage their societies, organizations and business or industrial connections to give liberally to the Joint Board's solicitation for funds for 1960. **John K. Taylor,** National Bureau of Standards.

SCIENCE AND DEVELOPMENT

The annual Tidal Current Tables for 1960 covering: 1. the Atlantic Coast of North America, 2. the Pacific Coast of North America and Asia, 3. Europe and the West Coast of Africa, including the Mediterranean Sea, 4. Central and Western Pacific Ocean and the Indian Ocean have been published by the Coast and Geodetic Survey of the U.S. Department of Commerce. Tide tables have been issued by the Survey since 1853. The tables are now published in four volumes which include the entire maritime world. They contain daily predictions for 190 reference stations and tidal differences and ranges at about 5,000 subordinate stations.

Culminating three years of exploratory work, **a plastic foam shelter** was demonstrated recently by the Atlantic Research Corporation (Arlington, Va.) and the Army Quartermaster Corps. The shelter, twelve feet in diameter and six feet high, was formed by spraying a rigid polyurethane plastic foam onto an inflated canvas hemisphere. The foam rises in a few minutes and hardens in less than an hour. The shelter weighs under 200 lbs. and is almost completely impervious to the elements. A better insulator than cork, the foam, one and a half inches thick, can easily be cut by a knife or bayonet. All components are portable making the foam shelter ideal for field use.

The National Science Foundation estimated in No. 16 of the series "Reviews of Data on Research and Development", that **total funds for scientific research and development in the U.S.A. will reach an all-time high of $12 billion** for the year 1959-60, up $7 billion from the 1953 level. An increase of nearly 160% in funds used in the performance of R&D by private firms and related organizations from 1953-54 to 1959-60 was found.

A highly efficient, large capacity "Fogger" has been developed by the Sanitary Engineering Branch of the U.S. Army Engineer Research and Development Laboratories, Fort Belvoir, Va. for use in control of mosquitoes and other flying insects. Using a solution of DDT the "fogger" can practically rid an area of insects in 10 minutes. In tests, fog was detected more than a mile from the point of release when the fog generator was held in a stationary position and operated at full output.

Modern mass education may be suppressing the development of "genius" claimed Dr. Harold G. McCurdy, University of North Carolina psychologist in a recent Smithsonian Institution report. In his study of the childhood of 20 generally admitted men of supreme ability, Dr. McCurdy found three factors common to all: 1. A high degree of attention focused on the child by parents and other adults, 2. Isolation from other children, especially outside the family, 3. A rich efflorescence of fantasy. The author remarked that "the mass education of our public school system is, in its way, a vast experiment on the effect of reducing all three of these factors to minimal value and should, accordingly, tend to suppress the occurrence of genius".

The history of science, invention and technology is featured in a new publication released in December by the Smithsonian Institution. The volume contains 11 research articles written by members of the Museum's staff. Alexander Graham Bell's part in making the phonograph a workable device is the subject of one article. Among other articles are ones that tell of the introduction of English patent medicines in colonial America, the failure of an early attempt to make a dollar watch, and the battle between Besse-

mer and certain of his contemporaries over patent rights to the process for producing cheap steel.

W. F. Libby, who served as a Research Associate in the Geophysical Laboratory at the Carnegie Institution of Washington during his entire term as Atomic Energy Commissioner, **spent the past year investigating the radioactive strontium content of rainfall** during the crucial period March-May, 1959, following an intensive series of bomb tests fired by the Soviet Union in October 1958. This event afforded a unique opportunity to test the characteristics of fallout from injections of nuclear materials at polar latitudes, as contrasted with the equatorial explosions fired by the U.S. and the United Kingdom. Analysis of samples collected during the critical period showed that the radioactive materials from the Russian shots had approximately one-year of residence in the stratosphere as contrasted with a residence time of three years or more for equatorially injected material. This discussion and many others appeared in the 1958-59 Annual Report of the President of the Institution.

The Optical Society of America has recently undertaken to translate the Russian journal, *Optika i Spektroscopiya,* with the help of a grant-in-aid from the National Science Foundation. This translation journal is being distributed free to OSA members as part of their membership privileges. It is also available to non-members in a package deal with the OSA *Journal,* at $25 per year.

"Preservation of Documents by Lamination", by W. K. Wilson and B. W. Forshee has been issued by the National Bureau of Standards (Monograph No. 5). Subjects covered include properties of an ideal laminating film; properties of cellulose acetate film; degradation of film and paper during lamination; preliminary studies of films other than cellulose acetate; and specifications for archival laminating film formulated from cellulose acetate. The study was requested and supported by the National Archives, U.S. Army Map Service, Library of Congress and Virginia State Library.

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Awards for Scientific Achievement	Frank A. Biberstein, Catholic University
Grants-in-aid for Research	B. D. Van Evera, George Washington University
Policy and Planning	Margaret Pittman, National Institutes of Health
Encouragement of Science Talent	Leo Schubert, American University
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